

DISCUSSION PAPER SERIES

IZA DP No. 16374

**Persistence of the Spillover Effects of
Violence and Educational Trajectories**

María Padilla-Romo
Cecilia Peluffo

AUGUST 2023

DISCUSSION PAPER SERIES

IZA DP No. 16374

Persistence of the Spillover Effects of Violence and Educational Trajectories

María Padilla-Romo

University of Tennessee and IZA

Cecilia Peluffo

University of Florida

AUGUST 2023

Any opinions expressed in this paper are those of the author(s) and not those of IZA. Research published in this series may include views on policy, but IZA takes no institutional policy positions. The IZA research network is committed to the IZA Guiding Principles of Research Integrity.

The IZA Institute of Labor Economics is an independent economic research institute that conducts research in labor economics and offers evidence-based policy advice on labor market issues. Supported by the Deutsche Post Foundation, IZA runs the world's largest network of economists, whose research aims to provide answers to the global labor market challenges of our time. Our key objective is to build bridges between academic research, policymakers and society.

IZA Discussion Papers often represent preliminary work and are circulated to encourage discussion. Citation of such a paper should account for its provisional character. A revised version may be available directly from the author.

ISSN: 2365-9793

IZA – Institute of Labor Economics

Schaumburg-Lippe-Straße 5–9
53113 Bonn, Germany

Phone: +49-228-3894-0
Email: publications@iza.org

www.iza.org

ABSTRACT

Persistence of the Spillover Effects of Violence and Educational Trajectories*

This paper provides evidence on how having violence-exposed peers who migrated to nonviolent areas affects students' educational trajectories in receiving schools. To recover our estimates, we exploit the variation in local violence across different municipalities in the context of Mexico's war on drugs and linked administrative records on students' educational trajectories. We find that peer exposure to violence in elementary school imposes persistent negative effects on students in nonviolent areas. Having elementary school violence-exposed peers has detrimental effects on students' academic performance in a high school admission exam and grade progression. For every ten students previously exposed to local violence who migrated to Mexico City's metro area, approximately five incumbent students in safe municipalities are placed in lower-ranked and less-preferred schools.

JEL Classification: I24, I25, O15

Keywords: local violence, peer effects, educational trajectories

Corresponding author:

María Padilla-Romo
Department of Economics
University of Tennessee
514 Stokely Management Center
Knoxville, TN 37916
USA
E-mail: mpadill3@utk.edu

* We are grateful to Xaber and Mexico's Ministry Education for providing the data used in this analysis. We thank Carolina Arteaga, Germán Bet, Felipe Barrera-Osorio, Ricardo Estrada, Caroline Fry, Susan Parker, David Sappington, and Rebecca Taylor; seminar participants at the University of Arizona, the University of Tennessee, the University of South Florida, the Universidad de Guadalajara, the Virtual Crime Economics seminar, and Centro de Investigación y Docencia Económica; workshop participants at the 2023 Leuven Summer Event, and the 2023 Development Economics in the South workshop; conference participants at the 2021 Southern Economic Association Annual Meeting, the 2022 Latin American and Caribbean Economic Association Annual Meeting, the 8th Congreso Anual de Economía y de Políticas Públicas Sobre México, and the 2023 Allied Social Science Associations Annual Meeting for helpful comments and suggestions on earlier versions of this paper. Any remaining errors are our own.

1 Introduction

Drug-related violence imposes high societal and economic costs in areas where violence occurs. These costs include deleterious effects on education (Brown and Velásquez, 2017; Koppensteiner and Menezes, 2021; Chang and Padilla-Romo, 2022), health (Koppensteiner and Manacorda, 2016; Brown, 2018; Lindo and Padilla-Romo, 2018; Martínez and Atuesta, 2018), and labor market outcomes (Montoya, 2016; Utar, 2018; Velásquez, 2019). Moreover, the detrimental effects of local violence spill over into safe areas via migration and peer exposure to violence in elementary school (Padilla-Romo and Peluffo, 2023). Yet, it is unclear if the spillover effects of local violence are short-lived or persist over time.

The adverse effects of peer exposure to local violence on academic performance in elementary school can shape students' academic trajectories. However, examining the persistence of the effects of peer exposure to local violence during elementary school is challenging due to the limited availability of data that follows students' performance over time or the lack of exogenous variation in peer exposure to local violence. This paper uses linked student-level data in a setting with extensive variation in plausible exogenous peer exposure to local violence to provide the first causal evidence of the longer-term spillover effects of local violence into nonviolent areas. Evaluating the persistence of these effects sheds light on the magnitude of the hidden costs that local violence imposes on society.

Our analysis takes place in the context of Mexico's war on drugs, which started at the end of 2006 and was characterized by a significant increase in local drug trafficking-related violence. Using the variation in homicide rates across Mexican municipalities and over time, Padilla-Romo and Peluffo (2023) show that local increases in violence act as shocks that induce individuals to move away from areas heavily exposed to violence into safer areas. Moreover, they show that this violence-induced migration generates negative spillover effects on the short-term academic achievement of incumbent students in safer areas. In this paper, we examine if exposure to peers who arrived from areas with high levels of violence and have been shown to be particularly disruptive in elementary school (low-achieving students from

violent areas) has long-lasting consequences for incumbent students in safer areas. While Padilla-Romo and Peluffo (2023) study the short-term effects of peer exposure to violence on a low-stakes diagnostic tests taken by students in elementary schools, this paper follows students who were exposed to potentially disruptive peers in elementary school over time using linked administrative data to examine the effects on students' academic trajectories. We offer a comprehensive analysis of the effects of peer exposure to violence, considering its impact on students' grade progression, academic achievement in high-stakes exams when applying to high school, high school placement, and consequent human capital misallocation, all of which are likely to affect incumbents' economic outcomes in the long run.

To identify how peers in elementary school affect students' academic trajectories, our empirical analysis exploits the quasi-random variation in exposure to peers from municipalities with high levels of violence across cohorts in a particular elementary school. Specifically, our regressions control for elementary school-by-grade fixed effects and leverage variation in the peer composition across cohorts, considering students' peer exposure in grades 4 to 6. In addition, the inclusion of year-by-grade fixed effects captures grade-specific common shocks by academic year. Given that exposure to local violence occurs before new peers arrive in their destination schools, the reflection problem (Manski, 1993) is unlikely to affect our estimates. That is, incumbents' academic trajectories in safer areas are expected to be unrelated to local violence in the municipality from which students migrate.

Our individual-level dataset contains linked information on elementary and middle school of enrollment, scores for diagnostic (low-stakes) tests taken in elementary school, demographic information, high school admission exam test scores, individual preference rankings over high schools for students taking the high school admission exam, age when taking the test, violence in each school location (measured by municipality homicide rates), and peer exposure to violence during elementary school. All students seeking admission to public high schools in Mexico City's metro area must take a high-stakes exam that (jointly with students' listed preferences over schools) determines their high school placement. To esti-

mate the effects of peer exposure to violence in elementary school on academic trajectories, we rely on the information on this high school admission exam and restrict our sample to incumbent students not exposed to local violence during elementary school.

Considering incumbent students in Mexico City’s metro area, we find that peer exposure to violence in elementary school imposes persistent negative effects on students in areas not experiencing high levels of violence.¹ Our estimates indicate that having a low-achieving peer who was previously exposed to violence in a classroom of 20 students in grades 4 to 6 of elementary school significantly reduces the performance in a high school placement exam by 4.7 percent of a standard deviation and harms grade progression. Our results imply that for every ten low-achieving students who migrated to the Mexico City metro area from violent municipalities, approximately five incumbent students in safe municipalities are misplaced (i.e., placed in high schools that are ranked below the high schools they would have been admitted to in the absence of peer exposure to violence). Because high-stakes exams are important determinants of labor market outcomes and achievement in the long run (Ebenstein et al., 2016; Machin et al., 2020), the human capital misallocation due to peer exposure to local violence is likely to induce additional negative effects on incumbent students later in life.

We implement a set of robustness exercises and show that the recovered estimates are robust to different specifications and identification threats. Considering potential selection into taking the high school admission exam, we follow Lee (2009)’s method to show that our estimates are robust to extreme assumptions on sample selection. Moreover, since there is variation in the timing in which incumbents across school-grades are exposed to peers from violent municipalities, we show that our conclusions are robust to using the Interaction-Weighted (IW) estimator developed by Sun and Abraham (2021).

This paper contributes to the literature on the effects of peer composition at different

¹We define a municipality as being violent if its homicide rate is above the 75th percentile of the distribution of homicide rates across municipalities. The definition of violent municipalities follows Padilla-Romo and Peluffo (2023).

school levels on long-term outcomes, which includes the impact on academic achievement, labor market outcomes, teenage pregnancy, and criminal behavior (Boozer et al., 1992; Rivkin, 2000; Gould et al., 2009; Bifulco et al., 2011; Black et al., 2013; Bifulco et al., 2014; Carrell et al., 2018; Anelli and Peri, 2019; Billings and Hoekstra, 2019; Abramitzky et al., 2021; Gazze et al., 2021). While the existing literature has studied the effects of different aspects of class composition on long-term outcomes, this is the first paper that estimates the long-term spillover effects considering peer exposure to local violence. The study by Carrell et al. (2018) for the U.S. is related to our analysis in that they study the persistence of peer effects, focusing on peer exposure to domestic violence. However, the type of violence that generates our peer effects differs substantially from within family violence (i.e., we are considering a context in which homicides are public, escalating, and the lack of safety expands within an area). Moreover, our study is the first to evaluate how peer exposure to violence affects performance in high-stakes exams later in life.²

The paper proceeds as follows. Section 2 describes the background, considering the particularities of the Mexican war on drugs, the education system in Mexico, and the high school admission process in Mexico City’s metro area. Section 3 describes the data we use in the analysis. Section 4 presents the identification strategies. Section 5 describes the main results. Section 6 presents a set of robustness checks. Section 7 concludes.

2 Background

2.1 Mexico’s Drug War

The fight against drug trafficking was identified as a priority during the administration of President Felipe Calderón. The Mexican war on drugs started in December 2006, during the second week of Calderón’s government. This process was characterized by attacks against

²Our paper is also related to the literature that examines the effects of violence on academic achievement in Mexico (Caudillo and Torche, 2014; Jarillo et al., 2016; Brown and Velásquez, 2017; Orraca-Romano, 2018; Chang and Padilla-Romo, 2022; Michaelsen and Salardi, 2020; Padilla-Romo and Peluffo, 2023).

Drug Trafficking Organizations (DTOs) led by federal troops and sought to restore safety in areas heavily affected by drug-trafficking violence. Figure 1 illustrates the evolution of the national homicide rates before and after the start of Felipe Calderón’s presidential term. The first operations in the context of the war on drugs, in which members of the federal police, the army, and the navy participated by confiscating drugs and capturing leaders of the DTOs, were followed by a temporary reduction in homicide rates. However, by 2008 violent crime increased, reaching levels comparable to those observed before the first operations started, and in the period 2009-2013, the homicide rate in Mexico achieved unprecedented levels.

The evolution of the national homicide rate (see Figure 1) masks significant heterogeneity in the expansion of violence across time and space. During this period, many municipalities remained safe while others experienced exponential increases in homicide rates. Our analysis uses the variation in the homicide rates across municipalities and over time to classify municipalities as violent and nonviolent. Considering the distribution of homicide rates in 2006-2013, we define a municipality as becoming violent in year t if, in that year, its homicide rate is above the upper quartile of the cross-municipalities distribution of homicide rates in the period 2006-2013.³ Our peer-effect estimates will focus on municipalities located in Mexico City’s metro area that remained nonviolent every year from 2006-2013 (we will refer to this group of municipalities as never-violent municipalities). As a reference, while the 25th percentile of the cross-municipalities average homicide rate was 4.05 homicides per 100,000 in the period, the 90th percentile was 38.18 homicides per 100,000 in the same period.⁴

³The definition of violent municipalities is the same as in Padilla-Romo and Peluffo (2023). This threshold is 18.01 homicides per 100,000 people.

⁴The average homicide rate in the municipalities included in our analytical sample was 7.6 homicides per 100,000 people in that period.

2.2 The Education System and High School Admission Process in Mexico City's Metro Area

The education system in Mexico has a basic education component that covers students of ages 3-14 years and comprises 1 to 3 years of preschool, six years of elementary school, and three years of middle school. After completing basic education, students can apply to enroll in high school, which typically requires students to take a high school admission exam.

High school consists of three years of education, and it is offered in two modalities: general and technical. Both general and technical high schools seek to prepare students to enroll in higher education. However, technical high schools also incorporate a subset of classes of vocational training that prepare students to join the labor market after graduation.⁵ Our primary analysis takes place in a centralized high school admission system in Mexico City's metro area, where nine school subsystems offer either general or technical high school education.

Before 1996, the high school admissions system in Mexico City's metro area was very inefficient. To increase their chances of being admitted to at least one public high school, students used to apply to multiple subsystems, each of which had its admission exam and selection criteria. When admitted to multiple high schools, students chose their preferred high schools and rejected other offers. In 1996, all public high school education subsystems in the metro area formed a consortium of public schools that aimed to address the aforementioned inefficiency in the admission system. They created a unique application and placement exam that has been carried out ever since by the *Metropolitan Commission of Public Institutions of Higher Secondary Education* (COMIPEMS).

Since 1996, every academic year, the admission process has started with the pre-registration period that takes place in February and March. During the pre-registration period, students submit a list of up to twenty schools ranked from the most to the least preferred and answer a

⁵For example, students receive vocational training in nursing, electricity, construction, and programming, among others.

context questionnaire with a wide range of information about the student. Students take the placement exam, which is the only determinant of high school admission, on the last weekend of June. Then, in July, using a computerized serial dictatorship mechanism (Abdulkadiroğlu and Sönmez, 1998), students are ranked from the highest to the lowest performer based on their test scores; the student at the top of the list is assigned to their most preferred high school with vacant spots. This process continues until all seats are taken, or no students remain in the queue.

3 Data

The data in the analysis come from administrative records from Mexico’s Ministry of Education, the National Population Council (CONAPO), and the National Institute of Statistics and Geography (INEGI) that combined yield a sample of students who took the National Assessment of Academic Achievement in Schools (ENLACE) exam between 2007 and 2013 and took the COMIPEMS exam between 2010 and 2018. These data contain information on students’ school location when taking low- and high-stakes exams, test scores, demographics, students’ preferences over high schools, and age when taking the high school admission exam, among other variables. We link these student-level data to each school’s municipality level of violence, measured by their homicide rates.

Municipalities’ homicide rates are constructed using the universe of death certificates from INEGI’s vital statistics and CONAPO’s population projections. For each academic year (August to July), we count the number of certificates with homicide as the presumed cause of death and transform this measure into per capita rates using municipalities’ population counts. We define municipalities as violent if their homicide rate is in the highest quartile of the cross-municipalities average homicide rate distribution from 2006 to 2013 (18.01 homicides per 100,000 people) and nonviolent otherwise. Figure 2 shows the location of violent municipalities and the academic year when they first became violent between 2006

and 2013. Overall, the map shows a large amount of variation in violence increases in geography and time, which we leverage in our identification strategy. Our peer-effects analysis takes place in never-violent municipalities in Mexico City’s metro area (shown in Figure 3).⁶

Using data from ENLACE, we build measures of migratory flows and baseline students’ achievement. ENLACE is a low-stakes diagnostic exam given to every student enrolled in public and private Mexican schools in grades 3 to 6 between 2006 and 2013.⁷ These data have information on math and reading test scores and the elementary school where each student is enrolled when they take the test. This information, coupled with the panel structure of ENLACE, allows us to identify whether and when students switch to a different school (relative to the previous academic year) and whether destination schools are located in the same or a different municipality. The school location choices, jointly with municipalities’ homicide rates, identify exposure to violence at the local level and indicate whether the municipalities of origin and destination are violent or nonviolent.

To identify how peer composition during elementary school affects outcomes later in life, we exploit the quasi-random variation in peer composition across cohorts in fourth to sixth-grade students in never-violent municipalities in Mexico City’s metro area. We define an intensity measure of the concentration of low-performing elementary-school peers previously exposed to local violence. That is, we calculate the share of peers in an elementary-school-grade-academic-year who migrated from a violent municipality and whose baseline test score is below the median of the school-grade-academic-year baseline test score distribution in the destination school.⁸ We focus on low-achieving peers previously exposed to violence because existing evidence for Mexico shows that this group of peers generates spillover effects on incumbents, as peer exposure to violence throughout high-achieving peers does not

⁶That is, municipalities in which every year between 2006 and 2013 the homicide rate was below the 75th percentile threshold.

⁷The attrition rate in the ENLACE exam is 12.1%. Padilla-Romo and Peluffo (2023) show that attrition in the exam is not correlated with whether or not and when a municipality is classified as violent or with the share of violence-exposed peers in a school grade.

⁸Throughout the paper, baseline test scores are the normalized sum of ENLACE’s math and reading scores the first time students take the ENLACE exam.

reduce incumbents’ achievement. Specifically, Padilla-Romo and Peluffo (2023) show that while peer exposure to violence via low-achieving students generates a large negative decline in incumbents’ academic achievement, exposure to high-achieving peers from violent areas slightly increases academic performance.⁹

For student i enrolled in grade g at elementary school s , in municipality m during academic year t , this share ($Share_{isgmt}$) is calculated as follows:

$$Share_{isgmt} = \frac{\sum_{h \neq i} Exposed_{hsgmt} * LowAchiever_{hsgt}}{n_{sgt} - 1}, \quad (1)$$

where n_{sgt} is the academic year t total enrollment at school s and grade g . The variable $Exposed_{hsgmt}$ equals one when a student h , who in year t is enrolled in grade g and school s (located in municipality m) was previously exposed to local violence, and zero otherwise.¹⁰ $LowAchiever_{hsgt}$ is an indicator of whether student h ’s baseline test score is below the median of grade g incumbents’ initial test score distribution at school s in academic year t .

Figure 4 shows the distribution of the number of low-achieving violence-exposed peers for incumbents in our main sample in school-grade-year combinations with at least one low-achieving student previously exposed to local violence.¹¹ In more than 70% of the cases, such incumbents only have one low-achieving peer previously exposed to violence, and in more than 90%, only one or two peers. Therefore, it is unlikely that the arrival of students from violent municipalities significantly changes school inputs per student.

Our main outcome variables are standardized COMIPEMS test scores and students’ age the first time they took the high school admission exam (a proxy for grade progression).¹²

⁹This positive effect is significantly smaller than the improvement that incumbents experience when exposed to high achievers who arrive in the school from other schools located in a nonviolent municipality.

¹⁰If students migrated more than once in our sample, they are classified as exposed to violence after they migrated from a violent municipality. We use the first municipality of origin to classify students who migrated more than once but were never exposed to violence (Section 4 explains how students who arrived from nonviolent municipalities are included in our analysis).

¹¹For presentation purposes, observations above the percentile 99 are excluded.

¹²16.04% of students in our main sample take the COMIPEMS exam more than once. We restrict our analysis to the first time they take the exam to avoid practice effects and avoid giving more weight to some students.

These variables come from the COMIPEMS exam and its context questionnaire. As this exam is taken at the end of middle school (or later), these data are typically observed (at least) between 3 and 5 years after students take ENLACE. COMIPEMS provides information on standardized high-stakes test scores, preferences for high schools, age, gender, parental education, the middle school where students were enrolled in ninth grade, and high school placement. Students' preferences are calculated considering the historic cutoff test score of the high schools the students listed as their first, second, and third choices.¹³

Using anonymized student identifiers, we link ENLACE and COMIPEMS records for each student over time. We restrict our sample to incumbent students: those who did not switch schools during elementary school and were enrolled in a middle school in Mexico City's metro area located in the same never-violent municipality (defined considering homicide rates in 2006-2013) in which they attended elementary school. Table 1 presents descriptive statistics for students in our sample, separately for those who were exposed to at least one peer from violent municipalities in grades 4-6 of elementary school and students who were not exposed to such peers. Approximately 32% of incumbent students in our sample were ever exposed to at least one low-achieving violence-exposed peer.

4 Identification Strategy

We recover long-term peer effects using a difference-in-differences specification. Our estimates compare outcomes for students in elementary-school cohorts with a relatively large share of low-achieving violence-exposed peers and students without a large share of classmates previously exposed to violence. Focusing on incumbent students in Mexico City's metro area, we identify the effects of exposure to low-achieving peers from violent municipalities in grades four through six of elementary school on high-stakes test scores (and age at test) after finishing middle school. Our sample includes incumbent students who, during elementary school, were located in never-violent municipalities. To avoid capturing poten-

¹³We construct a time-invariant cutoff for each high school by taking the average from 2010 to 2018.

tial adjustment costs to new local environments for students who switch municipalities, we restrict the sample to students whose municipality of elementary school enrollment is the same as their municipality of middle school enrollment.¹⁴

Since placement decisions across classes in a particular grade can be endogenous (for example, they may be explained by strategic decisions by principals or parents’ demands), we leverage the variation in the share of violence-exposed peers across cohorts in a particular grade rather than the variation at the class level.¹⁵ Our baseline specification is as follows:

$$y_{isgmt} = \lambda_{sg} + \eta_{gt} + \sigma_1 Share_{isgmt} + \beta X_{isgmt} + u_{isgmt}, \quad (2)$$

where y_{isgmt} is the COMIPEMS high school admission standardized test score (or age at test) for individual i , who was enrolled in grade g at elementary school s in municipality m in academic year t ; λ_{sg} is an indicator for the elementary school-grade in which the student took the ENLACE exam, and η_{gt} is a year of enrollment in grade g fixed effect. For student i enrolled in school s , the variable $Share_{isgmt}$ is the share of violence-exposed peers in grade g and whose academic achievement is below the median of the school grade year in which they arrive (as defined in Equation (1)); X_{isgmt} captures individual-level controls (i.e., baseline test scores); and u_{isgmt} is an error term that we allow to be correlated within elementary schools. In addition, our regressions include the share of new low-achieving peers who migrated from yet-to-be-violent and the share of new low-achieving peers from never-violent areas as controls. We weigh observations by the inverse of the number of years a student is observed in the sample.

In Equation (2), the parameter σ_1 recovers the causal effect of peer exposure to violence on incumbents’ high-stakes test scores (or age at test, a proxy for grade progression) provided that, conditional on school-grade invariant characteristics and time-variant con-

¹⁴Table A.2 in Appendix A shows that this restriction is not correlated with the share of peers exposed to violence.

¹⁵This approach is followed by Hoxby (2000); Lavy and Schlosser (2011); Abramitzky et al. (2021), among others.

trols, this share is not correlated with unobserved time-variant features at the school-grade level that also explain these outcomes.¹⁶ The validity of the identification strategy relies on the assumption that while selection into schools is plausible, the variation in the number of low-achieving peers previously exposed to violence in a particular cohort and grade is quasi-random. Intuitively, this implies assuming that without the arrival of students who were induced to migrate to safer areas due to local violence, the within-school performance of incumbents in cohort-grades with a large share of new violence-exposed peers would not have been systematically different from the performance of incumbents in cohort-grades with lower peer-exposure to violence, conditional on observable characteristics. When discussing our results, we will provide evidence supporting this assumption. Since local violence in the municipalities of origin is unlikely to be explained by incumbents’ academic achievement and progression later in life in the destination municipalities, the reflection problem (Manski, 1993) that typically affects peer effects estimates is not a concern in our setting.

In our empirical analysis, we observe students who take the COMIPEMS exam. However, peer exposure to violence in elementary school may affect the probability that incumbent students take the high school admission exam. We evaluate this issue and, in Section 6, we show that our estimates are robust to extreme assumptions on sample selection using the method proposed by Lee (2009). In addition, considering that students in different cohorts first became exposed to peers from violent municipalities in different academic years and the possibility of heterogeneous effects of peer exposure to violence across cohorts and over time, we estimate a slightly modified version of Equation (2) using the Interaction-Weighted (IW) estimator developed by Sun and Abraham (2021), which produces estimates that are robust to staggered treatments.¹⁷ In this case, and relying on the fact that more than 90% of school-grade-year combinations in which incumbents are exposed to low-achieving peers from

¹⁶As σ_1 captures the average effect of going from having no low-achieving peers exposed to violence to having all peers being low achievers and previously exposed to violence, we will provide interpretation taking into account that, typically, we observe a relatively small number of such peers arriving to a class-grade.

¹⁷Callaway et al. (2021) show that TWFE specifications in a dose-response context, such as ours, are not robust to dynamic and heterogeneous treatment effects across groups of students that were first exposed to peers from violent municipalities during different academic years.

violent municipalities have only one or two violence-exposed peers (see Figure 4), we replace our intensity measure, $Share_{isgmt}$, with a set of indicator variables for the years from the first arrival of a low-achieving violence-exposed peer. The IW estimates use never-exposed school grades as a comparison group, exclude always-treated school grades, and focus on the instantaneous effect (i.e., observations after the first arrival of a violence-exposed peer are dropped to avoid non-monotonic changes in treatment status). In this analysis, we also show that school-grade cohorts exposed to low-achieving peers from violent municipalities and never-exposed cohorts were in similar trends prior to the first arrival of low-achieving peers from violent municipalities, which provides additional support to our identification strategy.

5 Results

5.1 High-Stakes Test Scores

Our analysis begins by examining how peer exposure to local violence in elementary school affects students' performance later in life when taking the high-stakes high school admission exam (COMIPEMS). Considering incumbent students who were enrolled in elementary schools located in areas that were relatively safe,¹⁸ we focus on the effect of having elementary school low-achieving peers who were previously exposed to violence and migrated to safer areas. We build upon evidence in Padilla-Romo and Peluffo (2023), which shows that, in the short run, low-achieving new peers who were previously exposed to local violence generate large short-term declines in incumbent students' low-stakes test scores.

The estimated results are presented in Table 2. Column 1 contains the baseline specification in Equation (2). Column 2 adds gender and maternal education as controls. Column 3 presents our preferred specification, which additionally controls for state-by-grade-by-year

¹⁸Those students attended elementary schools in areas with homicide rates below the 75 percentile of the 2006-2013 cross-municipality homicide distribution (never-violent municipalities).

fixed effects to account for state-level changes that could differentially impact cohorts over time. The estimate of σ_1 shows that exposure to peers who arrived from violent municipalities in elementary school significantly reduces incumbents' performance in the high-stakes high school admission exam for students in Mexico City's metro area. The coefficient $\hat{\sigma}_1$ recovers the average effect of going from having no low-achieving violence-exposed peers in elementary school to being in a grade in which 100% of the peers are low-achieving students who arrived from violent places on incumbents' high school high-stakes exam test scores. In terms of a more typical classroom setting, these results indicate that, on average, having one low-achieving peer previously exposed to local violence in an elementary school class of 20 students reduces incumbents' performance in the COMIPEMS exam by 4.7 percent of a standard deviation. These estimates show that the effects of peer exposure to violence on cognitive outcomes persist over time.

The regression equations estimated in Table 2 include as controls the share of new low-achieving students who were not previously exposed to violence. Among those new low-achieving peers, we distinguish between students whose municipality of origin will become violent and those whose municipality of origin is never violent.¹⁹ Consistent with the literature on the connection between students' turnover and academic performance (Hanushek et al., 2004; Gibbons and Telhaj, 2011), Table 2 shows that the arrival of new peers reduces incumbents' achievement in the long run regardless of the origin of those new students. For example, these effects on incumbents can be attributed to disruptions that new students may generate in teaching and curriculum development (Hanushek et al., 2004). However, we find that the effect of being exposed to peers who arrived from violent municipalities is significantly larger than that of peers who were nonexposed to violence in their municipality of origin.

To shed light on how exposure to violence affects incumbent students' academic trajec-

¹⁹To define yet-to-be-violent and never-violent municipalities of origin, we consider whether or not the municipalities of origin have a homicide rate in the highest quartile of the cross-municipalities average homicide rate distribution between 2006 to 2013. Never-violent origin municipalities include students who switch schools within the same municipality and students who move across municipalities.

tories, a relevant exercise is to compare the effects of new low-achieving peers from violent municipalities with the effects of new low-achieving peers from municipalities that were not yet violent when students migrated. That is, having peers from the same origin municipalities before and after they first became violent, as those two groups of peers are likely to have similar previous experiences other than their exposure to local violence. We find that the effects of having peers previously exposed to local violence are 63% larger than those from municipalities that will become violent. These differences are not driven by selection in terms of student academic abilities, as there are no statistically significant differences in the baseline achievement levels between low-achieving students from violent and not yet-violent municipalities (see Figure A.1).²⁰ These estimates suggest that peer exposure to violence is an important mechanism driving the detrimental effects on incumbents' educational outcomes.²¹

5.2 Human Capital Misallocation

To better understand the long-lasting consequences on human capital development of having elementary school peers exposed to violence in their municipality of origin, we calculate the number of students who were misplaced in high school due to peer exposure to violence. We define misplacement as students being admitted to high schools that are lower ranked than the ones the students would have been assigned in the absence of violence-exposed low-achieving peers, considering their preferences for high schools in their priority list and counterfactual scores. We first calculate the counterfactual COMIPEMS test score using the estimated coefficients from our preferred specification in Column 3 of Table 2. We then compare this score to the cutoff scores of each school in students' priority lists. Overall, the estimates imply that for every 100 low-achieving students arriving from violent municipalities

²⁰Note that the estimated effects on baseline performance in Figure A.1 includes the same set of fixed effects as our preferred specification (i.e., school-by-grade fixed effects, grade-by-year fixed effects, and state-by-grade-by-year fixed effects).

²¹Low-achiever violence-exposed movers, however, have significantly higher baseline test scores than low-achieving students from never-violent municipalities (Figure A.1). This result is consistent with positive selection in out-migration from violent areas documented by Padilla-Romo and Peluffo (2023). Even in that case, the adverse effects of students previously exposed to violence on COMIPEMS test scores are significantly larger than those of students who arrived from never-violent municipalities.

to safe areas in Mexico City’s metro area, 2,189 students are ever-exposed to these peers, and 46 are misplaced (i.e., placed into a lower-ranked high school in their priority list).²² Marginal detrimental effects in test scores due to peer exposure to violence generate placement into a lower-ranked school which typically implies having, on average, lower-achieving peers and potentially less access to school inputs.²³

This counterfactual analysis assumes that incumbents’ ranked options are not affected by peer exposure to violence. However, peer exposure to violence can affect incumbents’ preferences and academic ambitions.²⁴ To examine this issue, Column 1 in Table A.1 shows the estimated effects on the number of high schools listed in the students’ priority lists. Columns 2, 3, and 4 present the estimated effects on their first, second, and third choices’ normalized average cutoff scores, respectively. Having low-achieving violence-exposed peers do not affect the number of high schools listed in the students’ priority lists. However, it does affect the type of high schools that students choose. The estimated effects in Columns 2-4 show that students with low-achieving peers from violent municipalities select high schools as their top choices with lower average cutoff scores than those without such peers. This result suggests that the counterfactual exercise provides a lower bound for the true effects on human capital misallocation.

5.3 Grade Progression

Grade retention may lead to dropout in certain contexts (Jacob and Lefgren, 2009; Manacorda, 2012) or reduce lifetime earnings due to delays in schooling completion. Given the detrimental effects found on cognitive outcomes and its potential impact on self-esteem and motivation, it is possible that having violence-exposed peers harmed students’ progression

²²In our sample, out of 641,594 students, 203,778 students are exposed to at least one of the 9,307 low-achieving peers who migrated from violent municipalities between 2007 and 2013, and 4,350 are misplaced.

²³In the context of Mexico City’s metro area, Estrada and Gignoux (2017) show that elite high schools increase the quality of education, measured by smaller classes, more college educated teachers, and fewer students per computer.

²⁴For example, the risk preferences of students who moved away from violent areas may have been shaped by local violence (Callen et al., 2014; Brown et al., 2019; Jakiela and Ozier, 2019). Social interactions with those peers can affect incumbents’ aspirations.

through elementary and middle school.

Considering this possibility, we estimate the effects of having low-achieving violence-exposed peers during elementary school on grade progression proxied by students' age when taking the COMIPEMS exam. The estimates in Table 3 show that having violence-exposed peers in elementary school harms grade progression for incumbent students. That is, it increases the age at which students take the high school admission exam for the first time. The arrival of a new low-achieving violence-exposed peer in a class of 20 students during elementary school increases incumbent students' age when taking the COMIPEMS exam by 0.017 years. The Table also shows the estimated coefficient for the share of low-achieving students that out-migrated from municipalities that will become violent in the future ($\hat{\sigma}_2$). The effects are close to zero and not significant at conventional levels when new peers come from yet-to-become-violent municipalities.

Taken together, our results imply that peer exposure to violence in elementary school can affect lifetime earnings, not just because of reducing the average quality of the high school in which incumbent students are admitted but also due to its adverse effects on grade progression.

5.4 Heterogeneous Effects by Grade of Exposure and Number of Peers

The richness of our data allows us to examine not just the average effect of peer exposure to violence in elementary school but also how peer exposure in distinct elementary school grades affects longer-term results allowing for potential heterogeneous effects in the number of violence exposed peers across elementary school grades. Specifically, we estimate a modified version of our preferred specification to allow for heterogeneous effects on test scores among students who were enrolled in fourth, fifth, and sixth grade; who were exposed to one, two, and three or more low-achieving peers who migrated from violent municipalities. That is, we interact indicators for the grade of exposure with indicators of the number of low-achieving

peers exposed to violence, as shown in the following equation:

$$y_{isgmt} = \lambda_{sg} + \eta_{gt} + \sum_{j=4}^6 \sum_{p=1}^{3+} \sigma_{jp} 1(g = j) \times 1(Peers_{isgmt} = p) + \beta X_{isgmt} + \epsilon_{isgmt} \quad (3)$$

where X_{isgmt} includes the same set of control variables as in Column 3 of Table 2, and $Peers_{isgmt}$ is the number of low-achieving violence-exposed peers of student i enrolled in school s and grade g in municipality m at academic year t . The coefficients of interest are σ_{jp} , and they measure the estimated effects on the COMIPEMS test scores of being exposed to p low-achieving violence-exposed peers in grade j .

The estimated results, presented in Figures 5 and 6, show that peer exposure to violence has stronger effects when exposure happens earlier in life (i.e., the effects are decreasing in the grade in which exposure occurs). Moreover, within each grade, the effects are increasing in the number of peers.

6 Robustness Checks

When analyzing the effects of peer exposure to violence during elementary schools on students' outcomes, our estimation sample includes students observed in elementary school and middle school who took the ENLACE and COMIPEMS exams at least once and did not switch schools (after taking ENLACE for the first time) in elementary school. Moreover, we restrict our sample to students who attended elementary and middle schools in the same nonviolent municipality (defined considering homicide rates in 2006-2013) and students born in the Mexico City metro area.²⁵ Considering all students who took the ENLACE exam in grades 4 to 6 in Mexico City metro area between 2006 and 2013, we examine how peer exposure to violence in elementary school affects the probability that a student is in our

²⁵We do not observe in which municipality students were born, but we do observe their state of birth. Considering that Mexico City's metro area is located in three states (i.e., Mexico City, Estado de México, and Hidalgo), we keep students born in either of these three states.

main estimation sample. Column 1 of Table A.2 shows that having low-achieving peers who arrived from violent municipalities in elementary school reduces the probability of being in the main estimation sample. Dropping out from the sample can be due to the decision to not take the high school admission exam or because of the sample restrictions imposed in our analysis. To investigate this issue further, in Column 2, we use the same sample as in Column 1 but define the outcome as an indicator for taking the COMIPEMS exam. The estimates suggest that the main driver for sample selection is the probability of not taking the high school admission exam and not the restrictions imposed in our sample. Restricting the sample to students who took the COMIPEMS exam, in Column 3 of Table A.2, we show that an indicator for being in the sample (i.e., fulfilling the sample restrictions) is not correlated with peer exposure to violence.

Considering that students with low-achieving violence-exposed peers are less likely to be in our estimation sample than non-exposed students (control group), we follow Lee (2009)'s approach and bound our estimates, making extreme assumptions regarding sample selection. Focusing on the distribution of the COMIPEMS test scores (and age at test), we assume that the additional individuals who take the COMIPEMS exam in the control group (relative to the treatment group) are either at the top or the bottom of the COMIPEMS test score (or age at test) distribution. Specifically, we drop 0.27% of students at the bottom and top of the COMIPEMS test score (or age at test) distribution of incumbents who did not receive low-achieving violence-exposed peers in their school-grade.²⁶ Tables A.3 and A.4 show that our main conclusions are robust to the potential endogeneity induced by sample selection with estimated effects on test scores between 4.4 and 5.4 percent of a standard deviations (and on age at test between 0.015 and 0.025 years) for students that receive one low-achieving violence-exposed peer in a class of 20 students.

In Section 4, we proposed an identification strategy that leverages variation in the concentration of violence-exposed peers across cohorts (Equation 2) to identify the long-term

²⁶This number is calculated by multiplying the average share of low-achieving violence-exposed peers of exposed incumbents with the estimated coefficient in Column 1 of Table A.2. That is, $0.021 \times 0.129 = 0.0027$.

effects of peer exposure to violence during elementary school in longer-term outcomes. In this section, we evaluate the existence of potential divergent trends in school-grades that received and did not receive immigrants from violent municipalities following the IW methodology proposed by Sun and Abraham (2021), which allows for treatment heterogeneity and dynamics in a context with unit staggered adoption. We define a school-grade as being treated after the first arrival of a low-achieving violence-exposed peer, use school-grades that never received violence-exposed peers as counterfactual, and drop always-treated school-grades.

Figure A.2 panels (a)-(c) present the estimated effects using the IW estimator. To frame the estimates on our different measures of educational trajectories, Panel (a) shows how the arrival of the first low-achieving peer from a violent municipality affects the concentration of such peers in a school grade. Panels (b) and (c) show the estimated effects on the COMIPEMS exam scores and age at the test, respectively. The estimates in Panel (a) indicate that the share of violence-exposed peers increases by two percentage points after the arrival of the first violence-exposed peer. The estimates in panels (b) and (c) show that before the first arrival of low-achieving peers from violent municipalities, longer-term students' outcomes in both groups of school grades follow a parallel path: the coefficients of having violence-exposed peers two or more years prior to treatment are not statistically significant and are close to zero, providing support to our identification strategy. Overall and consistent with our main results, having low-achieving violence-exposed peers decreases high-stakes test scores (by 2.16 percent of a standard deviation) and increases the age at the test (by 0.008 years).

For comparison, using the TWFE estimates in Column 3 of tables 2 and 3, increasing the share of violence-exposed peers by two percentage points decreases the COMIPEMS exam by 1.9 percent of a standard deviation and age at test by 0.007 years. We further allow our estimates to vary with the number of peers exposed to violence. Specifically, we compare never-treated incumbents, separately, to incumbents in school-grades that first received one, two, or three or more low-achieving peers exposed to violence. Figures A.3 and A.4 show

that, varying the intensity of treatment in terms of the number of peers previously exposed to violence, our main conclusions remain unchanged: the effects are increasing in the number of low-achieving violence-exposed peers and are not driven by pre-trends in the outcomes. These results indicate that the staggered treatment of school-grades does not threaten our identification strategy.

7 Conclusions

When estimating the costs that violence imposes on educational trajectories, spillover effects of local violence into relatively safe areas typically have been overlooked. In this paper, we show that the detrimental effects of local violence on students living in areas not directly affected by increases in homicides are persistent, large, and generate distortions in allocating human capital.

We find that having elementary school violence-exposed peers who out-migrated to safe areas negatively affects incumbents' academic performance in high-stakes admission exams and grade progression later in life. In Mexico City's metro area, the COMIPEMS exam is the only determinant of admission into public high schools. The detrimental effects on test scores imply reductions in admission probabilities to students' preferred schools. Taken together, our results indicate that the persistent spillover effects of violence into safer areas are hidden costs of violence that are likely to be exacerbated over time.

References

- Abdulkadirođlu, A. and Sönmez, T. (1998). Random serial dictatorship and the core from random endowments in house allocation problems. *Econometrica*, 66(3):689–701.
- Abramitzky, R., Lavy, V., and Pérez, S. (2021). The long-term spillover effects of changes in the return to schooling. *Journal of Public Economics*, 196(C).
- Anelli, M. and Peri, G. (2019). The Effects of High School Peers’ Gender on College Major, College Performance and Income. *Economic Journal*, 129(618):553–602.
- Bifulco, R., Fletcher, J. M., Oh, S. J., and Ross, S. L. (2014). Do high school peers have persistent effects on college attainment and other life outcomes? *Labour Economics*, 29(C):83–90.
- Bifulco, R., Fletcher, J. M., and Ross, S. L. (2011). The effect of classmate characteristics on post-secondary outcomes: Evidence from the add health. *American Economic Journal: Economic Policy*, 3(1):25–53.
- Billings, S. B. and Hoekstra, M. (2019). Schools, Neighborhoods, and the Long-Run Effect of Crime-Prone Peers. NBER Working Papers 25730, National Bureau of Economic Research, Inc.
- Black, S. E., Devereux, P. J., and Salvanes, K. G. (2013). Under Pressure? The Effect of Peers on Outcomes of Young Adults. *Journal of Labor Economics*, 31(1):119–153.
- Boozer, M. A., Kreuger, A. B., and Wolkon, S. (1992). Race and School Quality Since Brown v. Board of Education. *Brookings Papers on Economic Activity*, 23(1992 Micr):269–338.
- Brown, R. (2018). The mexican drug war and early-life health: the impact of violent crime on birth outcomes. *Demography*, 55(1):319–340.

- Brown, R., Montalva, V., Thomas, D., and Velásquez, A. (2019). Impact of Violent Crime on Risk Aversion: Evidence from the Mexican Drug War. *The Review of Economics and Statistics*, 101(5):892–904.
- Brown, R. and Velásquez, A. (2017). The effect of violent crime on the human capital accumulation of young adults. *Journal of Development Economics*, 127:1–12.
- Callaway, B., Goodman-Bacon, A., and Sant’Anna, P. H. (2021). Difference-in-differences with a continuous treatment. *arXiv preprint arXiv:2107.02637*.
- Callen, M., Isaqzadeh, M., Long, J. D., and Sprenger, C. (2014). Violence and risk preference: Experimental evidence from afghanistan. *American Economic Review*, 104(1):123–48.
- Carrell, S. E., Hoekstra, M., and Kuka, E. (2018). The long-run effects of disruptive peers. *American Economic Review*, 108(11):3377–3415.
- Caudillo, M. L. and Torche, F. (2014). Exposure to local homicides and early educational achievement in Mexico. *Sociology of Education*, 87(2):89–105.
- Chang, E. and Padilla-Romo, M. (2022). When crime comes to the neighborhood: Short-term shocks to student cognition and secondary consequences. *Journal of Labor Economics*.
- Ebenstein, A., Lavy, V., and Roth, S. (2016). The long-run economic consequences of high-stakes examinations: Evidence from transitory variation in pollution. *American Economic Journal: Applied Economics*, 8(4):36–65.
- Estrada, R. and Gignoux, J. (2017). Benefits to elite schools and the expected returns to education: Evidence from Mexico City. *European Economic Review*, 95:168–194.
- Gazze, L., Persico, C., and Spirovska, S. (2021). The long-run spillover effects of pollution: How exposure to lead affects everyone in the classroom. Technical report, National Bureau of Economic Research.

- Gibbons, S. and Telhaj, S. (2011). Pupil mobility and school disruption. *Journal of Public Economics*, 95(9):1156–1167. Special Issue: The Role of Firms in Tax Systems.
- Gould, E. D., Lavy, V., and Daniele Paserman, M. (2009). Does immigration affect the long-term educational outcomes of natives? Quasi-experimental evidence. *The Economic Journal*, 119(540):1243–1269.
- Hanushek, E. A., Kain, J. F., and Rivkin, S. G. (2004). Disruption versus tiebout improvement: The costs and benefits of switching schools. *Journal of public Economics*, 88(9-10):1721–1746.
- Hoxby, C. (2000). Peer effects in the classroom: Learning from gender and race variation. NBER Working Papers 7867, National Bureau of Economic Research, Inc.
- Jacob, B. A. and Lefgren, L. (2009). The effect of grade retention on high school completion. *American Economic Journal: Applied Economics*, 1(3):33–58.
- Jakiela, P. and Ozier, O. (2019). The Impact of Violence on Individual Risk Preferences: Evidence from a Natural Experiment. *The Review of Economics and Statistics*, 101(3):547–559.
- Jarillo, B., Magaloni, B., Franco, E., and Robles, G. (2016). How the Mexican drug war affects kids and schools? Evidence on effects and mechanisms. *International Journal of Educational Development*, 51(C):135–146.
- Koppensteiner, M. F. and Manacorda, M. (2016). Violence and birth outcomes: Evidence from homicides in brazil. *Journal of Development Economics*, 119:16–33.
- Koppensteiner, M. F. and Menezes, L. (2021). Violence and human capital investments. *Journal of Labor Economics*, 39(3):000–000.
- Lavy, V. and Schlosser, A. (2011). Mechanisms and impacts of gender peer effects at school. *American Economic Journal: Applied Economics*, 3(2):1–33.

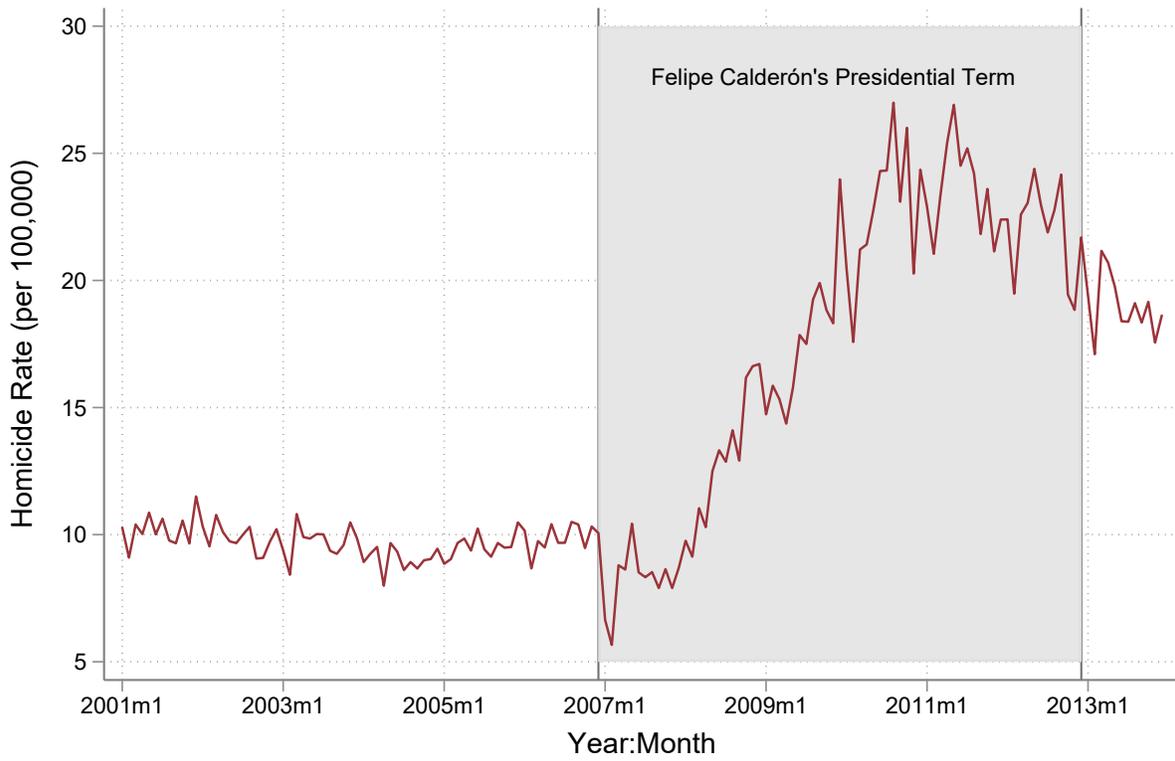
- Lee, D. S. (2009). Training, wages, and sample selection: Estimating sharp bounds on treatment effects. *Review of Economic Studies*, 76(3):1071–1102.
- Lindo, J. M. and Padilla-Romo, M. (2018). Kingpin approaches to fighting crime and community violence: Evidence from Mexico’s drug war. *Journal of Health Economics*, 58:253–268.
- Machin, S., McNally, S., and Ruiz-Valenzuela, J. (2020). Entry through the narrow door: The costs of just failing high stakes exams. *Journal of Public Economics*, 190:104224.
- Manacorda, M. (2012). The cost of grade retention. *The Review of Economics and Statistics*, 94(2):596–606.
- Manski, C. F. (1993). Identification of endogenous social effects: The reflection problem. *The Review of Economic Studies*, 60(3):531–542.
- Martínez, I. F. and Atuesta, L. H. (2018). Mourning our dead: The impact of mexico’s war on drugs on citizens’ depressive symptoms. *International Journal of Drug Policy*, 60:65–73.
- Michaelsen, M. M. and Salardi, P. (2020). Violence, psychological stress and educational performance during the “war on drugs” in Mexico. *Journal of Development Economics*, 143(C).
- Montoya, E. (2016). Violence and economic disruption: Firm-level evidence from mexico.
- Orraca-Romano, P. P. (2018). Crime exposure and educational outcomes in Mexico. *Ensayos Revista de Economía*, 0(2):177–212.
- Padilla-Romo, M. and Peluffo, C. (2023). Violence-induced migration and peer effects in academic performance. *Journal of Public Economics*, 217:2104778.
- Rivkin, S. G. (2000). School desegregation, academic attainment, and earnings. *The Journal of Human Resources*, 35(2):333–346.

Sun, L. and Abraham, S. (2021). Estimating dynamic treatment effects in event studies with heterogeneous treatment effects. *Journal of Econometrics*, 225(2):175–199.

Utar, H. (2018). Firms and labor in times of violence: Evidence from the mexican drug war.

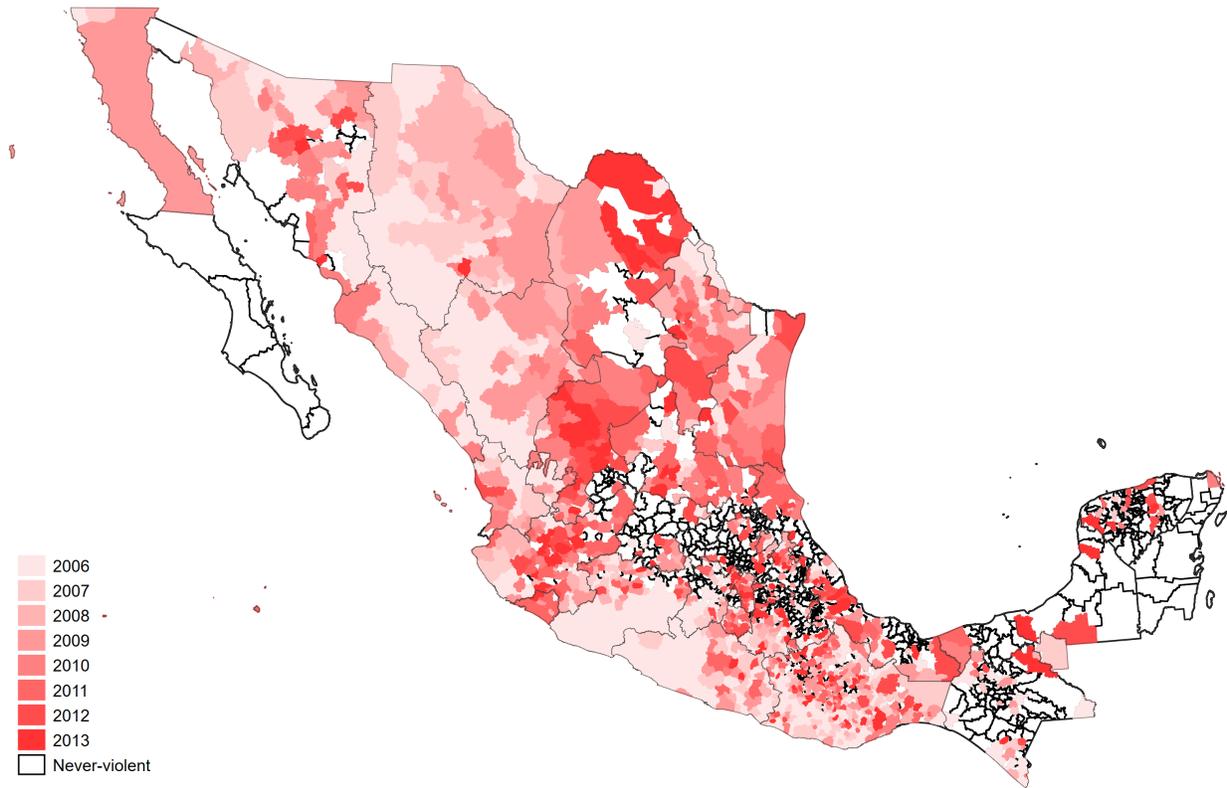
Velásquez, A. (2019). The economic burden of crime: Evidence from Mexico. *Journal of Human Resources*, pages 0716–8072r2.

Figure 1: National Annualized Monthly Homicide Rate per 100,000 People



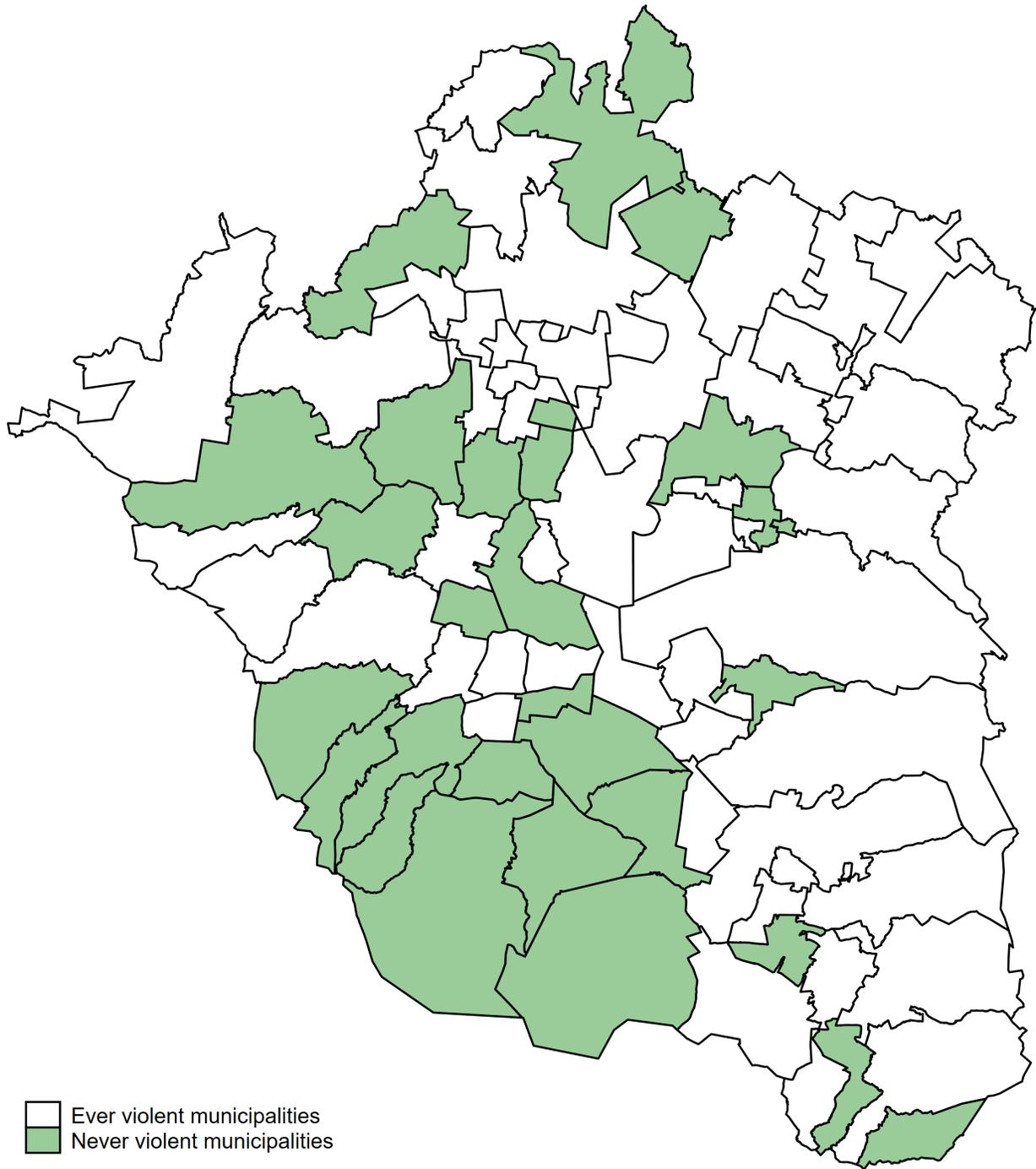
Notes: The vertical lines mark the beginning and the end of Felipe Calderón's presidential term.

Figure 2: Violent Municipalities by the Academic Year When They First Became Violent



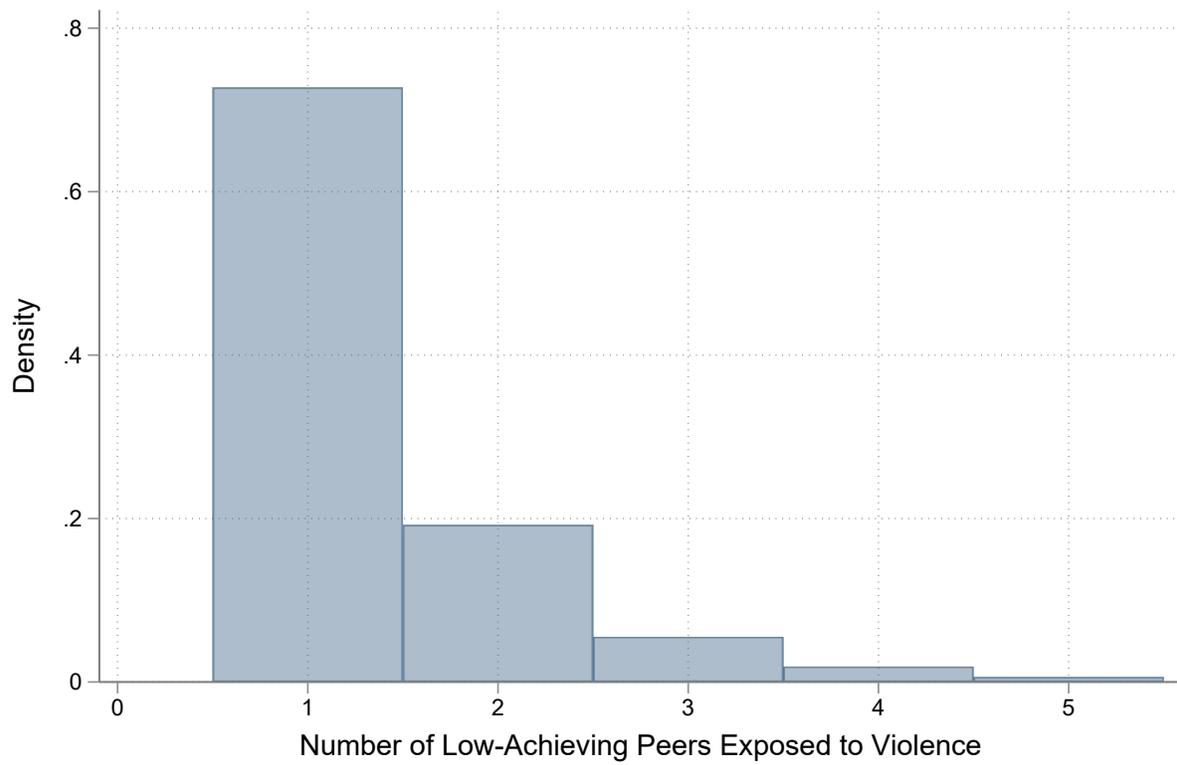
Notes: Municipalities that first became violent between 2006 and 2013. Municipalities are defined as violent if their homicide rate is in the highest quartile of the cross-municipalities average homicide rate distribution from 2006 to 2013 (18.01 homicides per 100,000 people).

Figure 3: Ever- and Never-Violent Municipalities in Mexico City's Metro Area



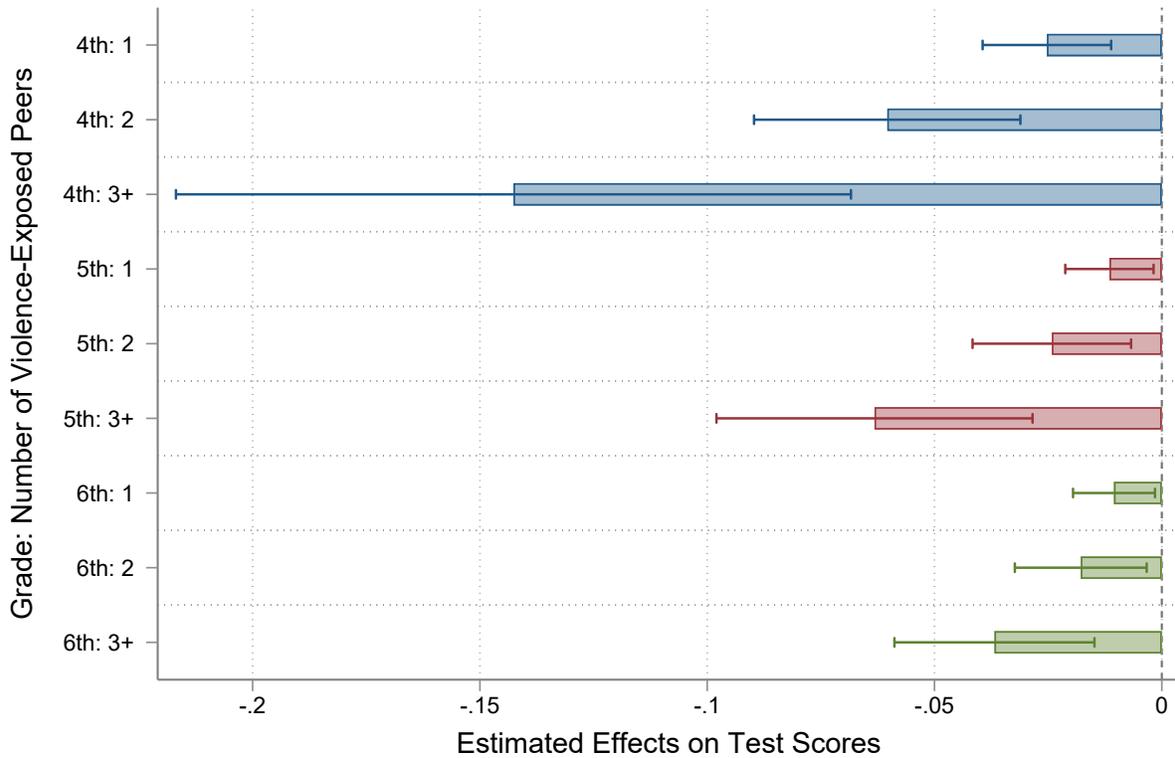
Notes: Municipalities that never became violent between 2006 and 2013. Municipalities are defined as never-violent if their homicide rate never was in the highest quartile of the cross-municipalities average homicide rate distribution from 2006 to 2013 (18.01 homicides per 100,000 people).

Figure 4: Distribution of the Number of Low-Achieving Violence-Exposed Peers



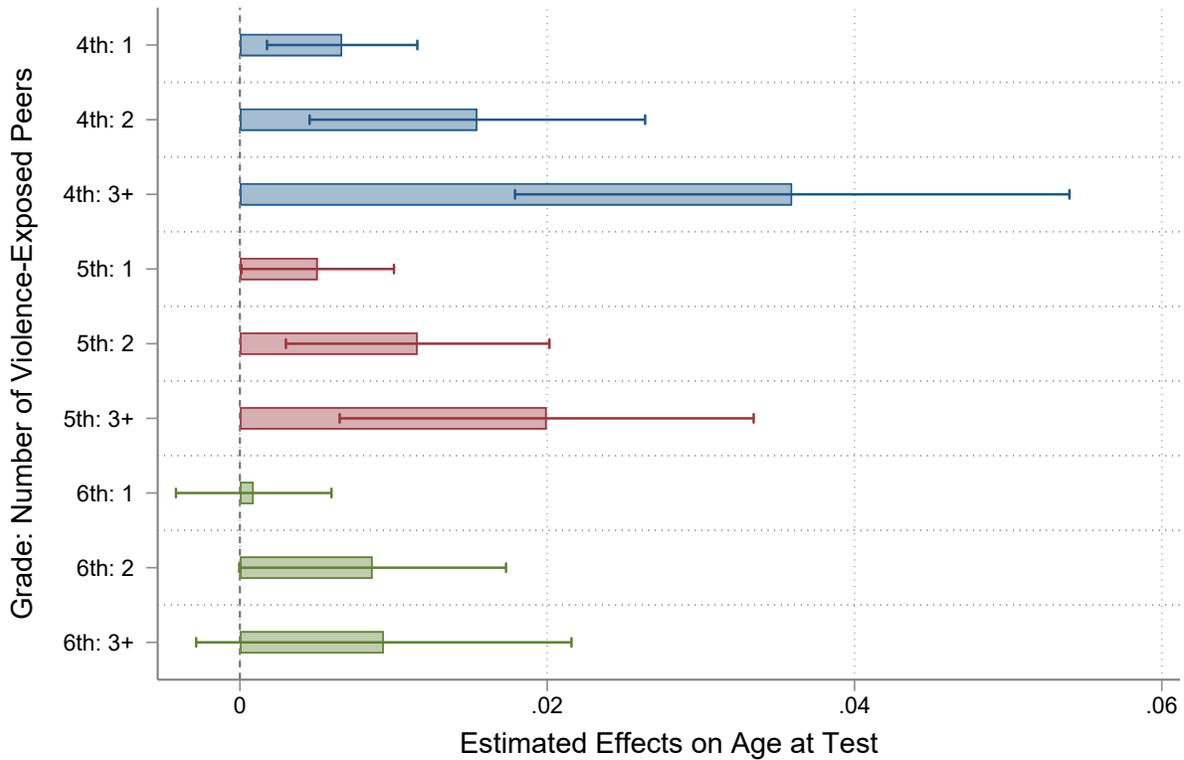
Notes: The figure only includes incumbents in school-grades (and years) with peers from violent municipalities. Observations above percentile 99 are excluded.

Figure 5: Estimated Effects on High-Stakes Test Scores by Grade and Number of Low-Achieving Violence-Exposed Peers



Notes: All estimates come from a single regression that includes baseline performance, the share of low-achieving peers from yet-to-become-violent municipalities, the share of low-achieving peers from nonviolent municipalities, school-by-grade fixed effects, grade-by-year fixed effects, state-by-grade-by-year fixed effects, and are weighted by the inverse of the number of years a student is observed in the sample. The regression also controls for indicators for gender, state of birth, and an indicator for maternal education being middle school or lower. Standard errors for confidence intervals are clustered at the school level.

Figure 6: Estimated Effects on Age at Test by Grade and Number of Low-Achieving Violence-Exposed Peers



Notes: All estimates come from a single regression that includes baseline performance, the share of low-achieving peers from yet-to-become-violent municipalities, the share of low-achieving peers from nonviolent municipalities, school-by-grade fixed effects, grade-by-year fixed effects, state-by-grade-by-year fixed effects, and are weighted by the inverse of the number of years a student is observed in the sample. The regression also controls for indicators for gender, state of birth, and an indicator for maternal education being middle school or lower. Standard errors for confidence intervals are clustered at the school level.

Table 1: Descriptive Statistics

	Ever-Exposed		Never-Exposed		All	
	Mean	SD	Mean	SD	Mean	SD
Female	0.505	(0.500)	0.507	(0.500)	0.506	(0.500)
Maternal Education: Middle-School or Lower	0.516	(0.500)	0.540	(0.498)	0.533	(0.499)
COMIPEMS Test Score	0.087	(1.028)	0.072	(1.036)	0.077	(1.034)
Age at Test	15.172	(0.485)	15.193	(0.503)	15.186	(0.497)
Ever Exposed to Low-Achieving Peers Yet to Be Exposed to Violence	0.369	(0.483)	0.218	(0.413)	0.266	(0.442)
Ever Exposed to Low-Achieving Peers Nonexposed to Violence	0.933	(0.250)	0.812	(0.391)	0.851	(0.357)
Took COMIPEMS on Time	0.950	(0.217)	0.950	(0.219)	0.950	(0.218)
Number of Options Listed	10.457	(4.072)	10.300	(3.938)	10.350	(3.982)
Average Cutoff: Option 1	-0.004	(0.997)	-0.006	(1.000)	-0.006	(0.999)
Average Cutoff: Option 2	-0.008	(0.998)	-0.004	(1.000)	-0.005	(0.999)
Average Cutoff: Option 3	-0.011	(0.994)	-0.007	(1.001)	-0.009	(0.999)
Obs.	485,443		875,065		1,360,508	

Notes: This table includes observations for students in our main sample. The means are weighted by the inverse of the number of years a student is observed in the sample.

Table 2: Estimated Effects of Low-Achieving Peers from Violent Municipalities on High-Stakes Test Scores

	(1)	(2)	(3)
Low-Achieving Peers from Violent Municipalities (σ_1)	-0.953*** (0.124)	-0.938*** (0.120)	-0.936*** (0.120)
Low-Achieving Peers Yet to Be Exposed to Violence (σ_2)	-0.583*** (0.118)	-0.578*** (0.116)	-0.575*** (0.116)
Low-Achieving Peers Nonexposed to Violence (σ_3)	-0.655*** (0.040)	-0.654*** (0.039)	-0.662*** (0.039)
Obs.	1,360,508	1,360,508	1,360,508
p-value ($\sigma_1 = \sigma_2$)	0.034	0.035	0.035
p-value ($\sigma_1 = \sigma_3$)	0.022	0.025	0.030
Additional student controls	no	yes	yes
State-by-grade-by-year fixed effects	no	no	yes

Notes: Each column represents a different regression. All regressions include baseline performance, school-by-grade fixed effects, grade-by-year fixed effects, and are weighted by the inverse of the number of years a student is observed in the sample. Additional student controls include indicators for gender, state of birth, and an indicator for maternal education being middle school or lower. Standard errors in parentheses are clustered at the school level.

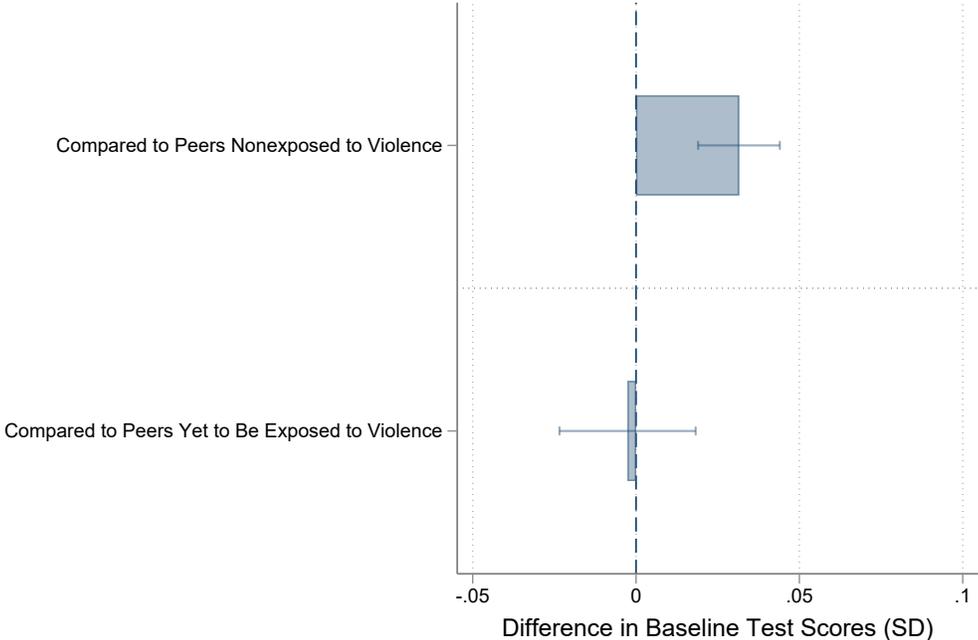
Table 3: Estimated Effects of Low-Achieving Peers from Violent Municipalities on Age at Test

	(1)	(2)	(3)
Low-Achieving Peers from Violent Municipalities (σ_1)	0.335*** (0.059)	0.323*** (0.058)	0.346*** (0.058)
Low-Achieving Peers Yet to Be Exposed to Violence (σ_2)	-0.033 (0.071)	-0.034 (0.071)	-0.032 (0.070)
Low-Achieving Peers Nonexposed to Violence (σ_3)	0.109*** (0.021)	0.102*** (0.021)	0.058*** (0.020)
Obs.	1,360,508	1,360,508	1,360,508
p-value ($\sigma_1 = \sigma_2$)	0.000	0.000	0.000
p-value ($\sigma_1 = \sigma_3$)	0.000	0.000	0.000
Additional student controls	no	yes	yes
State-by-grade-by-year fixed effects	no	no	yes

Notes: Each column represents a different regression. All regressions include baseline performance, school-by-grade fixed effects, grade-by-year fixed effects, and are weighted by the inverse of the number of years a student is observed in the sample. Additional student controls include indicators for gender, state of birth, and an indicator for maternal education being middle school or lower. Standard errors in parentheses are clustered at the school level.

Appendix A Additional Figures and Tables

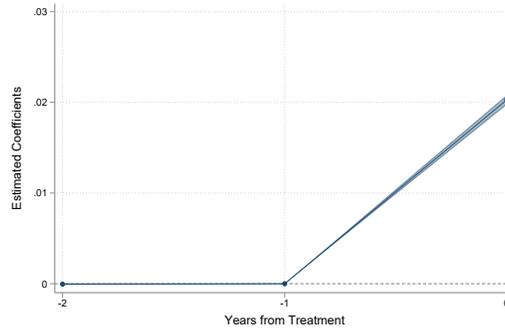
Figure A.1: Difference in Baseline Test Scores Between Movers from Violent vs. Nonviolent Municipalities



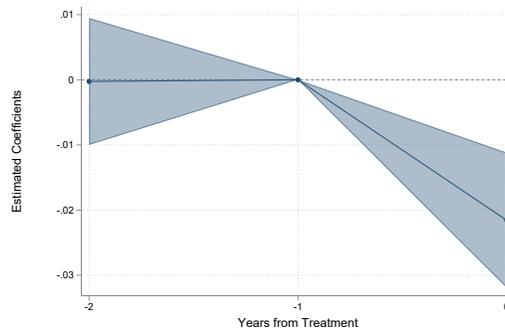
Notes: Each estimate comes from a different regression. Estimates include school-by-grade fixed effects, grade-by-year fixed effects, and state-by-grade-by-year fixed effects. Standard errors for confidence intervals are clustered at the elementary school level.

Figure A.2: Estimated Effects of Low-Achieving Peers from Violent Municipalities

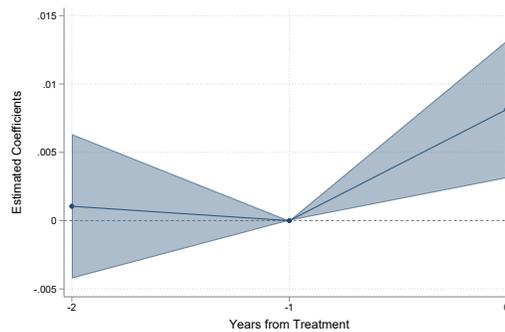
(a) Low-Achieving Violence-Exposed Peers



(b) High-Stakes Test Score

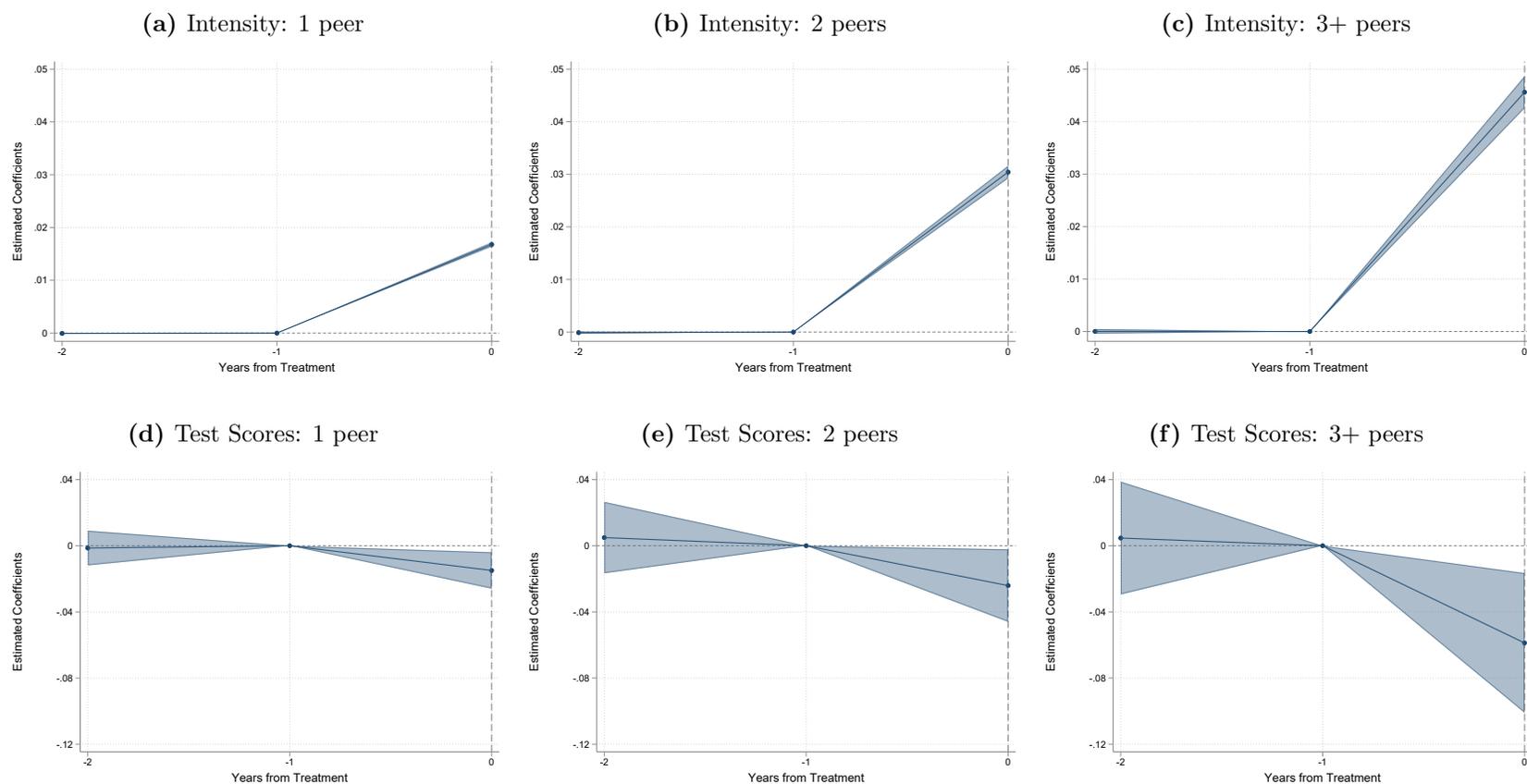


(c) Age at Test



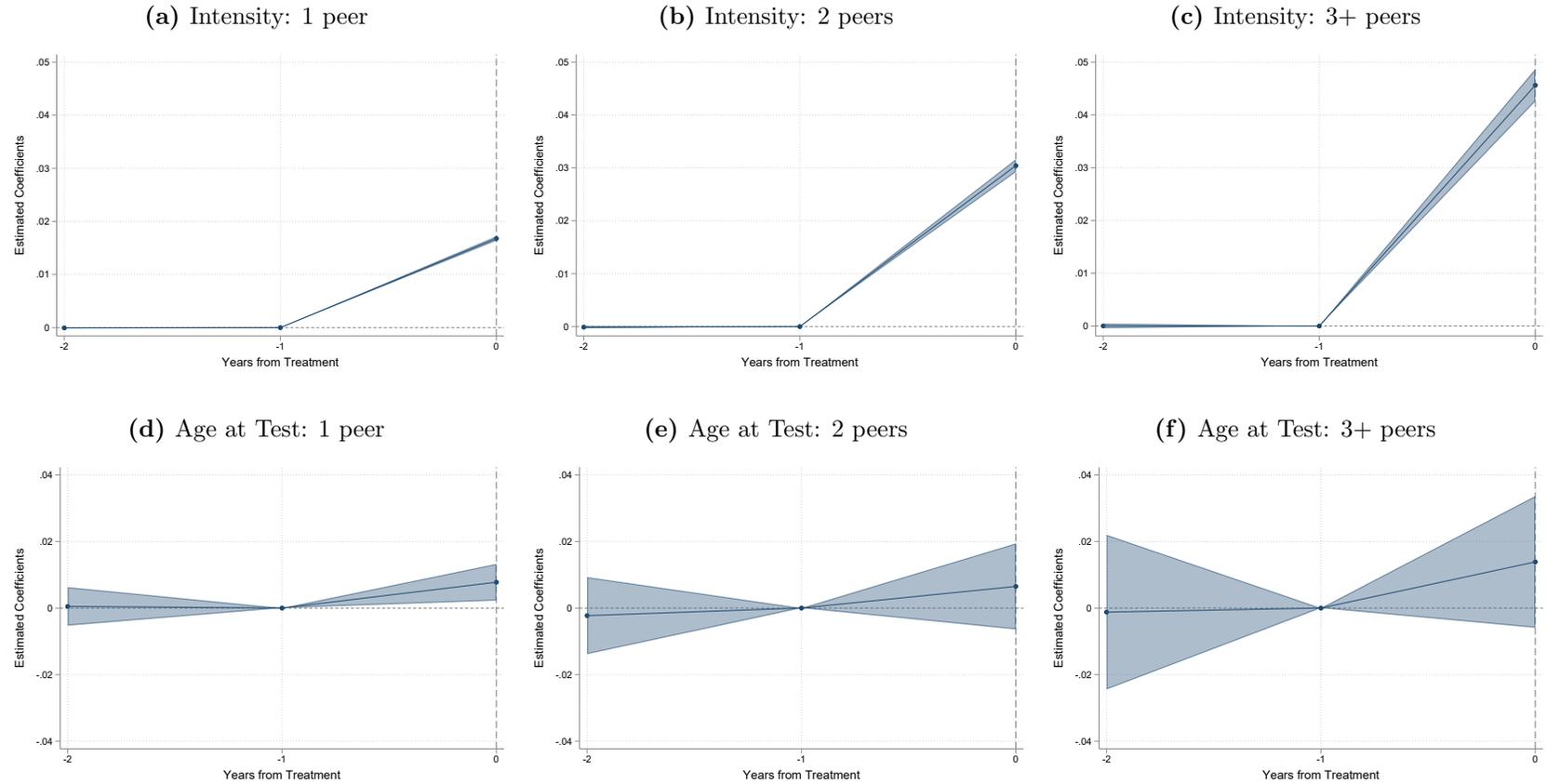
Notes: This figure shows estimated coefficients and their 95% confidence intervals for indicators for the years prior to and after a school grade first receives a low-achieving student from a violent municipality. All estimates are relative to the year prior to treatment. Estimates include baseline performance, the share of low-achieving peers from yet-to-become-violent municipalities, the share of low-achieving peers from nonviolent municipalities, school-by-grade fixed effects, grade-by-year fixed effects, and state-by-grade-by-year fixed effects. The regressions additionally control for indicators for gender, state of birth, and an indicator for maternal education being middle school or lower. Standard errors for confidence intervals are clustered at the school level. The estimates are obtained using the interaction-weighted estimator developed by Sun and Abraham (2021). Always-treated school grades are excluded from the analysis. Never-treated school grades are included in the control group. Observations are weighted by the inverse of the number of years a student is observed in the sample.

Figure A.3: Estimated Effects of Low-Achieving Peers from Violent Municipalities by Number of Peers



Notes: This figure shows estimated coefficients and their 95% confidence intervals for indicators for the years prior to and after a school grade first receives a low-achieving student from a violent municipality. All estimates are relative to the year prior to treatment. Estimates include baseline performance, the share of low-achieving peers from yet-to-become-violent municipalities, the share of low-achieving peers from nonviolent municipalities, school-by-grade fixed effects, grade-by-year fixed effects, and state-by-grade-by-year fixed effects. The regressions additionally control for indicators for gender, state of birth, and an indicator for maternal education being middle school or lower. Standard errors for confidence intervals are clustered at the school level. The estimates are obtained using the interaction-weighted estimator developed by Sun and Abraham (2021). Always-treated school grades are excluded from the analysis. Never-treated school grades are included in the control group. Observations are weighted by the inverse of the number of years a student is observed in the sample.

Figure A.4: Estimated Effects of Low-Achieving Peers from Violent Municipalities by Number of Peers



Notes: This figure shows estimated coefficients and their 95% confidence intervals for indicators for the years prior to and after a school grade first receives a low-achieving student from a violent municipality. All estimates are relative to the year prior to treatment. Estimates include baseline performance, the share of low-achieving peers from yet-to-become-violent municipalities, the share of low-achieving peers from nonviolent municipalities, school-by-grade fixed effects, grade-by-year fixed effects, and state-by-grade-by-year fixed effects. The regressions additionally control for indicators for gender, state of birth, and an indicator for maternal education being middle school or lower. Standard errors for confidence intervals are clustered at the school level. The estimates are obtained using the interaction-weighted estimator developed by Sun and Abraham (2021). Always-treated school grades are excluded from the analysis. Never-treated school grades are included in the control group. Observations are weighted by the inverse of the number of years a student is observed in the sample.

Table A.1: Estimated Effects of Low-Achieving Peers from Violent Municipalities on Students' Preferences

	Number of	Average Cutoff		
	Options Listed (1)	Option 1 (2)	Option 2 (3)	Option 3 (4)
Low-Achieving Peers from Violent Municipalities (σ_1)	-0.416 (0.473)	-0.245*** (0.091)	-0.197** (0.089)	-0.235*** (0.086)
Low-Achieving Peers Yet to Be Exposed to Violence (σ_2)	-0.087 (0.529)	-0.225** (0.110)	-0.330*** (0.111)	-0.160 (0.108)
Low-Achieving Peers Nonexposed to Violence (σ_3)	-0.060 (0.162)	-0.217*** (0.031)	-0.230*** (0.030)	-0.196*** (0.031)
Obs.	1,360,508	1,360,508	1,359,779	1,356,903
p-value ($\sigma_1 = \sigma_2$)	0.645	0.891	0.351	0.587
p-value ($\sigma_1 = \sigma_3$)	0.477	0.778	0.729	0.680
Additional student controls	yes	yes	yes	yes
State-by-grade-by-year fixed effects	yes	yes	yes	yes

Notes: Each column represents a different regression. All regressions include baseline performance, school-by-grade fixed effects, grade-by-year fixed effects, and are weighted by the inverse of the number of years a student is observed in the sample. Additional student controls include indicators for gender, state of birth, and an indicator for maternal education being middle school or lower. Standard errors in parentheses are clustered at the school level.

Table A.2: Estimated Effects of Low-Achieving Peers from Violent Municipalities on Sample Selection

	In Sample All (1)	Took COMIPEMS All (2)	In Sample Took COMIPEMS (3)
Low-Achieving Peers from Violent Municipalities (σ_1)	-0.129*** (0.044)	-0.134*** (0.045)	0.023 (0.045)
Low-Achieving Peers Yet to Be Exposed to Violence (σ_2)	-0.045 (0.044)	-0.131*** (0.045)	0.071 (0.048)
Low-Achieving Peers Nonexposed to Violence (σ_3)	-0.077*** (0.016)	-0.095*** (0.017)	-0.001 (0.016)
Obs.	2,632,839	2,632,839	1,816,025
p-value ($\sigma_1 = \sigma_2$)	0.173	0.968	0.470
p-value ($\sigma_1 = \sigma_3$)	0.264	0.424	0.614

Notes: Each column represents a different regression. All regressions include school-by-grade fixed effects, grade-by-year fixed effects, state-by-grade-by-year fixed effects, and are weighted by the inverse of the number of years a student is observed in the sample. Baseline performance and an indicator for female students are also included as controls. The outcome variable in Column 1 is an indicator equal to one if the student is in the main estimation sample and 0 otherwise. The outcome variable in Column 2 is an indicator for taking the COMIPEMS exam. Column 3 is restricted to students who took the COMIPEMS exam. The outcome variable in Column 3 is 1 if the student is in the main sample and 0 otherwise. Standard errors in parentheses are clustered at the school level.

Table A.3: Lee (2009)'s Bounds of the Estimated Effects of Low-Achieving Peers from Violent Municipalities on High-Stakes Test Scores

	Lower Bound (1)	Upper Bound (2)
Low-Achieving Peers from Violent Municipalities (σ_1)	-0.876*** (0.120)	-1.013*** (0.120)
Low-Achieving Peers Yet to Be Exposed to Violence (σ_2)	-0.569*** (0.115)	-0.574*** (0.115)
Low-Achieving Peers Nonexposed to Violence (σ_3)	-0.660*** (0.039)	-0.657*** (0.039)
Obs.	1,358,074	1,357,559
p-value ($\sigma_1 = \sigma_2$)	0.072	0.010
p-value ($\sigma_1 = \sigma_3$)	0.087	0.005

Notes: Each column represents a different regression. All regressions include baseline performance, school-by-grade fixed effects, grade-by-year fixed effects, and state-by-grade-by-year fixed effects. All regressions are weighted by the inverse of the number of years a student is observed in the sample. State of birth, an indicator for female students, and an indicator for maternal education being middle school or lower are included as controls. Standard errors in parentheses are clustered at the school level.

Table A.4: Lee (2009)'s Bounds of the Estimated Effects of Low-Achieving Peers from Violent Municipalities on Age at Test

	Lower Bound (1)	Upper Bound (2)
Low-Achieving Peers from Violent Municipalities (σ_1)	0.292*** (0.058)	0.499*** (0.057)
Low-Achieving Peers Yet to Be Exposed to Violence (σ_2)	-0.034 (0.070)	-0.021 (0.066)
Low-Achieving Peers Nonexposed to Violence (σ_3)	0.057*** (0.020)	0.053*** (0.019)
Obs.	1,357,504	1,357,802
p-value ($\sigma_1 = \sigma_2$)	0.000	0.000
p-value ($\sigma_1 = \sigma_3$)	0.000	0.000

Notes: Each column represents a different regression. All regressions include baseline performance, school-by-grade fixed effects, grade-by-year fixed effects, and state-by-grade-by-year fixed effects. All regressions are weighted by the inverse of the number of years a student is observed in the sample. State of birth, an indicator for female students, and an indicator for maternal education being middle school or lower are included as controls. Standard errors in parentheses are clustered at the school level.